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ORIGINAL COMMUNICATIONS.

THE AGE OF WOMEN WHEN THE CAPACITY FOR CHILD-BEARING CEASES.

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THE period when the capacity for child-bearing ceases in women is often a subject of medico-legal inquiry; yet, up to the present time, the question is left very loose and undetermined in the standard works on medical jurisprudence. I have several times been called upon to give an opinion on this point, and in this paper I shall give my reasons for expressing the belief that the limits of the capacity for conception and gestation in women can be defined with sufficient accuracy to warrant the medical expert in the expression of a positive declaration. Yet so many wonderful and apocryphal stories may be found in the medical literature of the past, which can be and often are quoted as disproving the accuracy of such a declaration, that it seems important to settle the question whether science can pronounce with authority that such pretended facts must be false. A zealous advocate, who feels that the interest of his client warrants him in bullying, by a rapid series of questions, in a loud tone of voice as if to intimidate from lying, may irritate and embarrass a medical witness to a degree that will destroy the force of a well-considered and sound opinion. For example, the medical expert is asked such questions as the following: "Do you set yourself up as an authority on this subject? Did you ever hear or read of Haller, one of the greatest lights in medicine that has lived in any age? Do you think that he made a false statement when he declared that he delivered one woman in her sixty-third and another in her seventieth year? Do you know anything of Tilt and Tanner as medical authors? What is your opinion of them? Now, will you say on your oath that they lie when they assert that Haller states that he did deliver two women at these ages? Did you ever hear of Dr. Benjamin Rush, one of the signers of the Declaration of Independence? Do you not think him rather a respectable man, who would be likely to speak the truth? Then are you ready to swear that he did not when he says that Ann Woods married after she was sixty years of age and gave birth to a child ten months afterwards? Did you ever hear of such a work as Beck's Medical Jurisprudence? Do you pretend that T. Romeyn Beck in this great work states an untruth when he declares that Ann Cook, of Whitehall, in this State, at the age of sixty-four had a child, born in February, 1836?"

A thorough knowledge of the literature of the subject and of the physiological laws which must govern it, an imperturbable temper, and great care in giving direct and simple answers to direct questions, and in avoiding the appearance of being a partisan advocate, will best protect the medical witness from the confusion and embarrassment of a false position.

Casper states "that every year cases come before us in which it is judicially inquired whether an aged woman, in her present marriage, or in a second one about to be entered into by her, may yet probably have children. Usually these are women approaching their fiftieth year, if they are not already far beyond it, and who have for long ceased to menstruate."* I suppose this question is a subject of judicial inquiry much less frequently in this country; yet every physician is liable to be called upon to testify in regard to this point. In two instances I have reason to believe that an opinion which I prepared at the request of eminent members of the legal profession served to avert scandalous litigation. In a third case, a woman said to be of weak mind and a confirmed opium-eater, wealthy by the rise of real estate in this city which she inherited, married a second husband when she was forty-nine years of age. She had no children by her first husband. At the age of fifty-eight she asserted that she was pregnant, and persisted in this assertion for nearly a year. Subsequently her husband and herself declared that she gave birth to a child, while in the country, during the summer, and a nurse and two other women were ready to swear to it, no physician being present at the time of the birth. Three months after this alleged delivery she was one morning found comatose, and died a few hours after, the death being reported as due to apoplexy. The following summer the child died from cholera infantum. The husband claimed the property which she left, as heir to her child. This claim was contested by two nephews and the husband of a niece of the woman. The counsel for the contestants submitted to me a great array of medical opinion, which purported to show, from numerous examples found in medical literature, that women over fifty-eight years of age have given birth to children. Before the case came to trial, conclusive evidence was found to establish the fact that the nurse, and not the old woman, was the mother of the child in question.

Within the past year I have been called upon to give testimony in a case involving a very considerable amount of property, the case turning upon the point whether it be possible for a woman after her sixty-seventh year to conceive and give birth to a child. It was with regret that I felt it my duty to give an opinion, and my reasons therefor, adverse to that of several of my respected confrères.

My object in the present paper is to show that physiology and experience have determined the limits of the reproductive functions in women. It is generally assumed that the duration of the child-bearing capacity is coincident with the duration of menstruation. Most writers on forensic medicine, and most medical witnesses in the cases in which

* Forensic Medicine. The New Sydenham Society, London, vol. iii. p. 260.

I have been engaged, have referred to the numerous examples of menstruation prolonged beyond the usual periods of life as evidence that such women are capable of impregnation and gestation.

While it is generally true that when menstruation ceases the ovaries are no longer capable of furnishing mature ova for fecundation, and the uterus no longer has the power of retaining and developing an ovum, yet, on the one hand, thousands of sterile women menstruate regularly and normally, while, on the other hand, there are great numbers of women who have conceived before they have menstruated, and others who have given birth to a child some years after menstruation has ceased. Many cases, whose authenticity is beyond question, can be found in medical literature which demonstrate the truth of both assertions. I can furnish such which have come under my personal observation.

A lady in perfect health, except that she never had menstruated, was married at twenty. In the ten years following her marriage, I attended her in eight confinements. In 1870 I received a letter from her written in Switzerland, where the family have been living for some years for the purpose of educating their children, saying that she had not been exposed to the danger of having more children since the birth of her last child, then six years old; but that her husband was endeavoring to persuade her that she must be now safe, as she never had been unwell. I wrote in reply that, as she never had menstruated before the birth of her children, I regarded her as an unsafe subject for me to commit myself to an opinion on this point.

Another lady in this city, previous to her marriage, menstruated very scantily and irregularly. But in the twelve years since her marriage she has never menstruated, but she has given birth to ten children, and she is still keeping up her good work.

As an example that conception and gestation may occur some years after menstruation has ceased, I will mention the case of a lady in this city who had borne five children, the last at the age of thirty-eight. She ceased to menstruate at forty-two. Her health had previously been delicate, but she now became strong and well, and increased in weight over thirty pounds. When more than forty-six, I was asked to examine her, as she had felt many of the usual symptoms of pregnancy. I expressed the opinion that she was five and a half months pregnant; and my diagnosis proved correct, as in due time she gave birth to the finest and largest child that she had ever borne. In this case there was no motive for deception, and the result was a matter of family pride and rejoicing.

In November, 1851, I was invited by Professor Willard Parker to see a very curious and interesting case, which had been examined by several obstetricians. A woman, whose character was good so far as known, five years a widow, represented her age as forty-seven, and that she had not menstruated for three years. An abdominal tumor could be felt, reaching to the level of the umbilicus. An incessant convulsive movement of the abdominal integuments and muscles (phenomena sometimes, but rarely, associated with pregnancy, described by Velpeau,* but hardly alluded to by any other obstetrical writer) rendered obstetric auscultation quite impossible and abdominal palpation wholly unsatisfactory. The mammary signs of pregnancy were negative. This woman gave birth to a child, supposed to be a little premature, the following March, and soon after died of puerperal fever.

Ovulation and menstruation, although often coincident, do not necessarily bear the relation of cause and effect, as has been supposed by many. Ritchie† and others have demonstrated by autopsies that ovulation does not always occur at the time of menstruation. Peaslee‡ remarks, "There is no proof and no probability that ovulation produces true menstruation. Certainly, ovulation, and even parturition, occurs in some women who have never menstruated. Menstruation is therefore an accidental and an incidental rather than an essential function, and it has no analogue in most of the lower animals. In itself considered, it is merely a flow of blood at stated periods from the interior of the uterus, irrespective of its connections and its causation."

Ovulation is absolutely an essential function for conception and gestation; but menstruation is not. Hence the numerous cases reported by medical writers, and quoted in works on medical jurisprudence, of a regular recurrent sanguineous flow from the genital organs in women over fifty-five years of age, no more demonstrate the possibility of conception in such women than does the same apparent menstrual discharge in women who have had both ovaries removed, of which there are several authentic cases.

When that physiological process takes place known as senile atrophy of the ovaries,—that is, when the ovaries diminish in volume, shrink up, and become wrinkled, the Graafian follicles disappear, the stroma becomes dense and non-vascular,—then ovulation ceases, and conception and gestation are no longer possible. All physiologists are in agreement in fixing the period of this change in woman as between forty and fifty. In a very exceptional number of women this change does not take place until one, two, three, or even four years later. This is proved not only by thousands of autopsical examinations, but also by the fact that not a single authentic case is known, of the slightest scientific value, where a woman over fifty-five years of age has given birth to a child, with the exception of Sarah, the wife of Abraham, in biblical history. Abraham was one hundred and Sarah was ninety years old when Isaac was born; and it is said "it ceased to be with Sarah after the manner of women." It is not surprising that Sarah, when she heard the announcement of her future pregnancy, "laughed within herself, saying, After I am waxed old shall I have pleasure, my lord being old also?" According to Scott's Commentary, "the name Isaac signifies laughter, and this child of promise was so named in remembrance of Abraham's believing and Sarah's unbelieving laughter."

From the time of Pliny down to the present day,

* Meigs's Velpeau's Midwifery, Phil., 1852, p. 397.
† Ovarian Physiology and Pathology, London, 1865.
‡ Ovarian Tumors, New York, 1872, p. 529.

there have been many cases reported in medical literature and quoted in works on forensic medicine, where women from fifty-eight up to eighty years of age are said to have given birth to a child.

But in a scientific inquiry I hold that we are bound to reject all statements which are opposed to the universal experience of mankind, when the probabilities against the truth of the assertion are at least as a million to one, unless it be supported by such evidence as would be regarded as conclusive in a court of law. Some of these reported cases have their only foundation in either tradition, travellers' tales, common rumor, or an anonymous paragraph in a newspaper or a magazine. Others again arise from a misquotation or a perversion of the original statement. The limits of this paper will not permit me to trace to the original source and analyze all of these cases, but I will briefly examine those which seem to have been accepted generally by writers on medical jurisprudence and by some recent obstetrical writers.

One of the earliest cases alluded to by many is given on the authority of "Pliny the Naturalist." Montgomery* says, "Pliny records the case of Cornelia, of the family of Scipio, who at the age of sixty bore a son, who was named Volusius Saturninus." Tilt† makes a somewhat different statement, as follows: "Cornelia was confined of Valerius Saturnius in her sixty-second year." It is sufficient to say, in regard to this, that the eldest child of Cornelia was born 163 B.C., and that Pliny was born 23 A.D. Consequently, one hundred and fifty years at least must have elapsed from the time of this extraordinary birth to the time when Pliny wrote; and, as he gives no documentary evidence in proof of his statement, it is safe to infer that it was derived solely from tradition.

Numerous writers assert that Massa, a physician of Venice (the name is variously spelled when quoted, as Marsa, Marra, Mansa), says "that he treated a woman for dropsy, who was really pregnant, he being deceived by her age, which was sixty." The writer referred to must be Nicholas Massa, of Venice, who died 1569. In his work,‡ in the "Epistola de aqua cutem seu hydrop," he speaks of a case in which a woman of sixty was treated for dropsy when she was pregnant, but he does not say that he was personally cognizant of the fact.

Many obstetrical and forensic authors refer to Valescus de Tarenta as "mentioning a woman who continued to menstruate beyond sixty, at which age she bore her last child." Valescus de Tarenta was a native of Portugal, born in 1382, and practised medicine in Montpellier, France. His work, "Practica quæ alias Philonium dicitur," can be found in that remarkable and valuable collection of medical books, the library of the New York Hospital, and perhaps some one who can read mediæval Latin with facility may take the trouble to ascertain exactly what he did say.

The weight of the great name of Haller has been invoked in proof of the fact that women of advanced age have borne children. I find in many of the standard writers on this subject the assertion that "Haller delivered one woman in her sixty-third and one in her seventieth year." This curiously illustrates how books are filled with hasty, erroneous, and perverted quotations, for there is no evidence that Haller ever practised midwifery. He was born in Bern, 1708. In 1735 he was appointed physician to the hospital in his native town, but in 1736, when he was twenty-eight years of age, he was made a professor in the University of Göttingen, and from this time he gave up the practice of his profession. All that Haller ever said in his writings on this subject is as follows: "Women have been seen to bring forth in their fifty-fourth year, fifty-eighth, sixtieth, sixty-third, seventieth, and among my relatives was a child-bearing matron whose two sons born after their mother's fiftieth year, chosen into the senate, are living at the time of my writing this."§

Capenon is often quoted as an authority that women of an advanced age have borne children. But the only personal statement which he makes on this point is "that in Paris it passes for true that a woman in the street *La Harpe* gave birth to a daughter at the age of sixty-three."‖ On which Casper makes the following comments: "Those acquainted with Paris well know the class of people living there: small shop-keepers, tradesmen, and the like. Are these observations possessed of the slightest scientific authenticity?"¶

The case of Ann Woods, who is said by Dr. Benjamin Rush "to have given birth to a child after she was sixty years old," is often referred to; but the evidence of the truth of the story rests entirely upon the assertion of the old woman herself, "who called at his house to beg for cold victuals," and at this time she claimed to be ninety-six.**

Another famous case, often quoted, is that of Ann Cook, of Whitehall, who is said to have had a child at the age of sixty-four.†† This story originated in an anonymous communication to the *Boston Medical and Surgical Journal*,‡‡ and is entirely unsupported by any evidence.

I think it wholly unnecessary for me to refer to various other reports of aged women who have given birth to children, which rest solely on paragraphs in newspapers and magazines, as these are of no scientific value.

After the most careful and laborious research, I can find but one authentic case, based on the evidence of a respectable medical man, who carefully investigated the documentary proof of the fact, where a woman who has reached the age of fifty-five has given birth to a child. This case is reported by Dr. Davies, of Hertford, England.§§

* Cyclopædia of Practical Medicine, London, vol. iii. p. 491; also Montgomery on Pregnancy, Phil., 1857, p. 260.
† Change of Life, 2d ed., London, p. 18.
‡ Epistolæ Medicinales, Venet., 1549.

§ Elementa Physiologiæ, Bernæ, 1765, tomus vii. pars ii. p. 141. I give the passage in the original, with the authorities which Haller cites: "Peperisse visæ sunt feminæ anno 54(c), 58(d), 60(e), 63(f). 70(g), et inter consanguineas meas fuit patritia matrona, cujus duo filii post quinquagesimum matris annum nati in senatum adlecti supervivunt dum hæc scribo." (c) Eph. Nat. Om., vol. vii. obs. 80. (d)Hist. de la Chirurg. (e)Plot. Natur. Hist. of Oxford. Valesc. de Tarenta. (f)Plot., Wallace, p. 64. (g Salmut, Cent. iii. obs. 59.
‖ Médecine Légale, Paris, p. 93.
¶ Forensic Medicine. The New Sydenham Society, vol. iii. p. 260.
** Rush's Essays, Philadelphia, 1798, p. 301.
†† Beck's Medical Jurisprudence, 10th ed., p. 246.
‡‡ Vol. xiv. p. 79. §§ London Medical Gazette, vol. xxxix.

There are a considerable number of cases which must be accepted as authentic, as the evidence is conclusive, that women between the ages of fifty and fifty-four have borne children. I have personally known three women over fifty years of age who have become mothers. One case, which came under my observation, is so curious that I may be pardoned for relating it, as it is well known to many now living in this city.

May 6, 1852, I attended a case of labor in St. Mark's Place, in consultation with the late Dr. Robson of this city. The labor was normal but tedious, and our patient was delivered of a daughter by the aid of the forceps. This lady had been married twenty-seven years, and this was her first pregnancy. After the birth of the child, the husband showed to Dr. Robson and myself a family Bible, in which the birth of his wife was recorded as having been May 5, 1801.

July 3, 1853, Dr. Robson having died, I attended this lady in her second confinement. The mother and both daughters (now married) are still living.*

The question as to the age when the capacity for child-bearing ceases has been frequently brought before courts of law. In one instance only, so far as I can learn, it was decided that this is possible at the age of fifty-eight. François Fajat claimed an estate as heir to his mother. His claim was resisted on the ground that, according to the baptismal registry, his mother could not have been the legitimate heiress through whom the claim accrued, because her alleged mother would have been in her fifty-eighth year, and this, it was contended, was beyond the age of child-bearing. The court referred the case to the Academy, who, from the "Annals of Medicine," produced the apocryphal cases which I have before examined. The court admitted the legitimacy on the grounds that *menstruation* and conception had been known to occur at periods even later than this.† This was in Paris, in 1754.

The legitimacy of the claimant in the famous Douglas Peerage case, whose alleged mother at the time of his birth was in her fiftieth year, was decided in his favor. Until within a recent period, the English law has admitted "no presumption as to the time when a woman ceases to have children, though this enters into most other codes." ‡ But at the present time the fact is otherwise. In the English Court of Chancery, the succession to a large property depended entirely on the question whether a woman at sixty years of age might have a child. The Attorney-General, Sir William Horne, stated that there was no such case satisfactorily recorded; and he offered to give up his client's title if any credible evidence could be produced in support of an instance. But, as none was brought forward, he was deemed to have succeeded in proving his claim.§

The English Court of Chancery seem now to have arrived at a definite conclusion as to the age at which a woman may be presumed to be past child-bearing. *In re* Widows' Trusts, Vice-Chancellor Malino made an order for payment to two ladies respectively. One of the ladies was a widow, past fifty-five, the other a spinster, aged fifty-three years and eight months. In both cases the parties were entitled absolutely, subject to the contingency of having children.‖

In Forty *vs.* Forty (Feb. 1853), Vice-Chancellor Kindersley decided that an unmarried lady aged fifty-three might be presumed to be beyond the age of child-bearing. Taylor remarks, "The question may be considered definitively settled. A woman who has passed the age of fifty-three is presumed in law to be past the age of child-bearing." ¶

I am not aware that this question has ever been judicially settled in our American courts. But, in conclusion, I feel warranted in stating the proposition that the laws of physiology, the experience of mankind, and the decisions of courts of law, justify a medical witness in declaring that a woman over fifty-five years of age is past the period of child-bearing.

* The husband, whose age was fifty-four when his first child was born, was for many years captain of a New York and Liverpool packet-ship. I asked him how he explained the curious fact that his wife had now for the first time become a mother. His answer was that "he had followed the sea for many years, until he had got money enough to live as comfortably as he wished, when he concluded to stay at home and 'tend to his family duties."

† Devergie, Médecine Légale, tom. i. p. 435; also Capenon, Médecine Légale, p. 93.

‡ Beck's Medical Jurisprudence, vol. i. p. 653.

§ Tanner, Signs of Pregnancy, London, 1860, p. 18; also London Medical and Surgical Journal, vol. iii. p. 687.

‖ 40 Law Journal Rep., N. S., 380.

¶ Taylor's Principles and Practice of Medical Jurisprudence, London, 1873, vol. ii. p. 306.

TETANUS NASCENTIUM.

BY W. H. WINSLOW, M.D.

A WELL-DEVELOPED, rosy, hearty male infant, six days old, awoke at midnight, screaming, and the mother, in attempting to quiet him, found that he could neither hold on to, nor nurse from, the nipple; and, upon further examination, she discovered that the child had its lower jaw fixed, and its limbs rigid. The mother and several "old women" worked with him during the remainder of the night and the following day, trying to force down breast-milk, teas, and cure-alls, without success. Everything flowed from the mouth, and spasm after spasm finally awoke a spasm of sense, and started them after a doctor. I saw him at 6 P.M.

The women reported that the child had been delivered by a midwife, had had proper urinary and fecal evacuations every day, and had been perfectly well up to the time of the sudden nocturnal attack.

The parents occupied a very small room, with one window and door, in the second and upper story of a house upon the southwest side of which the heat on that July day was very oppressive, even after sunset. The perspiration ran from my face in streams while I made the examination. In this room had occurred her accouchement. Below was a low grog-shop; around the house the gutters were full of stinking slops and kitchen débris, and the Schuyl-

kill, at no great distance, hinted of malarial influence. The dietetic supplies for the mother were coarse and inappropriate, and she, having passed her labor without mishap, had abandoned her bed on the fifth day. She was a blonde, with freckled, sallow, pock-marked face, and large blue eyes, of medium size, tolerably well developed, but one whose form, bride as she was of a year, spoke of tangents and secants,—*i.e.*, angular. Her age was twenty-two years.

The husband, one year her senior, was of medium size, brown hair and eyes, and most powerfully developed,—a condition which his labor as carter promoted and maintained. His health was perfect, but he had lost several fingers of his right hand by a circular saw in March, which accident had greatly shocked and distressed his wife,—then five months pregnant,—and had diminished their resources somewhat.

The first thing that attracted attention to the patient was the peculiar facial expression, and the intensely congested condition of the whole surface of the body. The skin was dry and hot. He lay upon his back, with head retracted, and eyelids so firmly closed as decidedly to resist attempts to separate them. The pupils were dilated, and did not respond to light; the lips were pressed firmly against the gums, making the mouth a slit about a quarter of an inch wide in the middle; the corners of the mouth were slightly drawn downwards, the tongue was pressed between the gums anteriorly, and foaming saliva filled the aperture, constantly flowing inwards and outwards during inspiration and expiration.

The lower jaw was so firmly fixed by its muscles that I could not move it by exerting all the force I dared to use. A space existed between the gums just sufficient to permit the introduction of the end of my index-finger. It seemed just wide enough to contain the teeth, had they been developed.

The patient's bowels had not been moved for eighteen hours, and he had not taken any nourishment since the beginning of the attack. The nostrils were dilated and immovable. The arms were drawn inwards, so that the elbows were within the marginal line of the body and could not be drawn outwards by the exercise of much force; the forearms were strongly pronated, flexed at an angle of 130° on the humeri; the hands were slightly extended, abducted, and brought nearly together at the middle line, with the fingers clenched and the thumbs *outside* of the fingers. This position of the thumbs is normal during the latter part of intra-uterine life, and is not departed from during the earlier weeks of external existence, because it requires time for the flexors to relax from their constrained ante-partum condition, and for the extensors to gain power to overcome them and produce equilibrium. The positions of the arms reminded me strongly of the pugilistic attitudes of children when they first mimic their elders.

The thorax was elevated and rounded, as at the end of inspiration, and remained so; while the abdomen was projected and tympanitic. The thighs were only slightly flexed on the pelvis, and the legs on the thighs. The feet were extended extremely, and strongly abducted, and the inner malleoli were almost in contact. The great toes were abducted from the other toes, which were flexed, but their first phalanges were extended on their metatarsi.

Some one has said that the arm should be studied with the palm upon the ground, and pronated, in a position representing the fore-leg of a quadruped. If this patient's arms had been so placed, we would have found, upon comparison with the legs, that corresponding groups of muscles in the two members were affected in like manner and degree, and that in those groups which are most used and most powerful in life the tension seemed greater, while, all the muscles being contracted, the positions of the bones (other things not interfering) indicated the balance of power,—the resultant of all the forces.

The muscles were everywhere in tonic contraction, yet every moment there would occur a quick jerk, a letting-go-and-taking-hold-again movement, apparently from renewed nerve-irritation, which caused more rigid tension. If this movement were to be represented in diagram, it would not differ much from that of the healthy pulse as indicated by sphygmographic tracings. The infant's respiration was 50 per minute, very short and superficial; the expiration instantaneous, and he struggled for breath with his lungs full of air, like a patient with severe emphysema. The pulse was 140 per minute, thready, or, rather, corded, and distinct. He could not close his lips around the nipple, or swallow any liquid from a teaspoon, and every attempt with either only increased the violence of the spasms. He cried in an irregular whining manner, and remained in the spasms for ten minutes. Upon examining the umbilicus, the cause of the mischief was immediately apparent. There existed a hernia about the size of an English walnut, with thin walls, much congested, the redness forming an areola around the retracted and inflamed cicatrix of the funis. Pressure applied to the umbilicus only elicited most distressing cries and a more rigid contraction of the muscles. From careful examination I felt certain that the inflammation extended along the vessels of the cord inwards. Upon reducing the hernia, the spasms ceased immediately; and upon letting the projection occur again it immediately supervened. Here was an important fact in reference to the etiology of the disease, as well as a hint to the doctor. I kept the hernia reduced, and put him to the breast. The moment his lips were pressed against the moist nipple, the spasm returned again. A salt-and-water injection brought away a yellow stool of moderate quantity; a warm bath was given; the umbilicus was dressed with cerat. plumbi and carbolic acid upon a pad, kept in place by a bandage, to keep the hernia reduced. After this the spasm yielded, and a little milk was swallowed from a teaspoon several times, when suddenly a puff of wind from a rising storm came in at the window upon the little patient, and he went off into another spasm. Three powders, each containing res. scammonii gr. ⅛, and hydrarg. chl. mit. gr. ¼, with sugar, were ordered,

one to be given every two hours, and potass. brom. gr. ii, in solution, were directed to be given every alternate two hours.

The next morning, at nine o'clock, I found the patient had been convulsed part of the night, had not slept, but had taken medicines, and some milk from the spoon. He could not close his mouth upon the nipple, and the attempt intensified the trismus. Bowels had moved freely. The facial expression and spasmodic condition were the same. Cheeks were cool and cyanosed, the cerulean hue somewhat mixed with erythema of the skin extended over the trunk. Sudaminæ over the neck and breast. Umbilicus felt and looked cooler and better. Respiration was 90 and the pulse 150 per minute. Ordered ext. physostigmæ, gr. $\frac{1}{48}$, sod. sulphit. and quin. sulph. āā gr. i, in solution, to be given every two hours; ungt. belladonnæ to be applied to the spine, and the nourishment with brandy to be pressed. The third day friends reported that the patient had been very bad all the day before, but had passed a quieter night, and taken medicine and food as directed. General appearance was much the same. The intervals between the spasms were more frequent and more prolonged. The cyanosis was more marked over the extremities, and the cries were more feeble. Respiration was 106 and pulse 180 per minute. Hernia was still reduced, and doing well. When the child was put to the breast, spasm came on, but milk given in the spoon was sometimes swallowed, sometimes rejected. The tetanic symptoms were better, but patient was becoming weaker. During the spasms a white fluid (milk) was regurgitated from the stomach freely, so that what was gained between spasms was lost during their continuance. Treatment was continued, but the physostigma was increased to gr. $\frac{1}{24}$ every two hours.

The fourth day I found the child in convulsions, which had been very bad since the previous evening, so that no nourishment or medicine could be taken. Fæces and urine had been voided during the night. Face looked old, wrinkled, and blue. The surface of the body everywhere was sodden and dark from venous congestion. The jactitations of the muscles kept fists moving comically. Limbs resisted forcible attempts to change their positions. Arms drawn outwards a little, sprang together again when released. Umbilicus looked almost well, but I squeezed out a drop of pus from below the cicatrix. Oral aperture was filled by saliva and the point of the tongue. Eyelids were closed, but pupils dilated. Respiration was 130 and pulse 200 per minute; the latter determined imperfectly by ear over the heart. Continuous convulsions existed till 5 P.M., when the child died. Parents would not permit a post-mortem. These cases being so rare, I took my notes by the bedside, that the data might be reliable. I desire attention to the following summary. The bad hygienic conditions surrounding the case, the fright of the mother at the fifth month, and the pathological condition of the cord, may all be regarded as causes: the first two predisposing, the last one exciting. The attack came on between the third and tenth days, which is the rule. The convulsions affected the whole of the muscles at once. There was little opisthotonos, because the ante-partum flexion had not yet been counterbalanced. There was eagerness but inability to take the breast, except during the relaxations or intervals; and even then the act seemed to excite spasm quicker than any other. Food and medicine were given and retained during the earlier intervals, but regurgitated during spasms later. Pulse was accelerated when first seen, and increased much as the disease progressed; contrary to the statements of authorities, "that it is diminished." The bones of the head were not displaced, and no diarrhœa occurred. The whole duration of the disease was eighty-nine hours. When the hernia protruded, convulsions came on immediately; when the tumor was reduced, they temporarily ceased: this, in connection with the inflammation of umbilical vessels and the drop of pus, excludes pyæmic and centric causes, and brings out clearly peripheric irritation as the principal factor. It will be noticed that my therapeutics were directed to all three. My next case I shall treat in the main locally.

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REPORT OF AN OPERATION FOR LACERATED PERINEUM—UNION FOLLOWING FAILURE OF THE ANAL SUTURE.

BY J. J. MAGUIGAN, M.D.

MRS. McF., a healthy young woman, 23 years of age, was confined in a small village in the State of Michigan, at full term. The presentation was cephalic, occiput front. During the long, difficult parturition the perineum gave way with a violent pain which expelled the head, the rent extending through the sphincters into the rectum. The child was dead.

The accident entailed incontinence of flatus and fæces. With this disgusting infirmity upon her she again became pregnant. In due time I delivered her in this city, without accident, of a healthy male child.

Six weeks after the parturition, I undertook the operation, in which I was kindly assisted by Drs. Wilson, Buckley, and James A. Maguigan. Her bowels were unloaded the preceding day with a dose of castor-oil, after which a two-grain pill of opium was given. Being thoroughly etherized, she was placed in the usual lithotomy position. The operation was rendered extremely difficult by the absence of one of the attendants expected, and was fitfully and hurriedly performed. The vaginal surface of the septum was freely denuded of its mucous covering for an inch of its extent. The lacerated surfaces were next pared off, the incision commencing below and taking off a thin border from the labium. In breadth it extended to the vaginal mucous membrane. Three sutures of silver wire were employed. The needle, armed with the first suture, was made to enter three-fourths of an inch from the margin of the wound just below its lowest point, and, traversing its entire depth, was brought out on the middle of the septum, just above the line of denudation. The needle, being now disarmed, was withdrawn and made to enter on the opposite side at a corresponding point and to emerge on the septum close to the first. Now, the evident design of this thread is to draw tightly together the vaginal septum and the oval extremity of the wound and hold them in this close contact until the resulting

inflammation unites the parts in one and restores the integrity of the sphincter ani. This is its grand aim and function. Great importance is consequently attached to the proper disposition of this suture. But the authority which teaches that the fate of the operation hangs on this stitch, that if union be primarily defeated here, though the rest of the wound have united throughout, the operation cannot be viewed in any light other than that of an absolute failure, is an error. The truth of this proposition, which is not unfairly stated, will be exemplified in the sequel. The two other sutures were placed equidistant, the last near the top of the wound. The parts were closely approximated, and the opposite ends of the wire were brought together and secured by compressed shot. Interrupted sutures were superficially deposited between them. The operation was now finished. The external aspect was handsome. I explored the interior, and, to my chagrin, found a communication between the anus and the vagina. Across the chasm, stretched hard like the string of a bow, the wire could be distinctly felt. I saw that in my haste I had on one side (the left half) passed the wire directly across the vaginal face of the septum, instead of behind it as I was pleased to declare in a foregoing sentence. I feared for the success of the operation, but was not inclined to open up the wound again. I satisfied myself with the hope that the opening would close up by granulation during the progress of the inflammation.

I attended to the after-treatment myself, with the most scrupulous exactness in its many details. I drew off her urine with a catheter twice daily for the first few days; after this she relieved herself by turning forward on her side. I syringed the parts daily with a detergent composed of carbolic acid and water. Her bowels were kept quiet with opium. Nutritious food—beef-tea, eggs, and milk, articles which leave little residuary matter to fill up the bowels—was freely administered. Her knees were confined with a bandage. I had the good will and co-operation of the patient, and the support and encouragement of her sensible mother. Everything indeed seemed to work in the most harmonious accord, with a hopeful promise.

On the sixth day, finding that the sutures had outlived their usefulness, I removed the two anterior ones and found that the union was here complete. I let the posterior stitch remain until the following day, but as it was then clearly of no further service I removed it. The wound gaped at the anal aperture, and revealed a channel of communication between the anus and the vagina large enough to admit the point of the finger. Here there was certainly a failure of the first stitch, and consequently of the operation. I hesitated to communicate the mortifying intelligence to my patient. I stated my case to Philadelphia's great perineal surgeon. He gave me little hope. I requested him to examine the case before passing judgment. This he kindly consented to do. After exploring the parts, he declared in the most positive manner that the result was destitute of any value, and that the whole perineum would have to be opened up again, and advised a new operation. With this crust of comfort he left me. Now, to my mind (as well as to my eye) it seemed clear that the operation which I had done had resolved a bad case of perineal laceration into one of simple anal fistula with division of the sphincter ani. I recognized the case only in its new relations, and treated it as the primary and essential lesion. I touched the fistulous track with a crayon of nitrate of silver. I returned in a week, and found, to my delight, that the track was nearly closed. One more touch with the *lapis mirabilis*, and the parts came nicely together and cicatrized. The patient's recovery was simultaneously proclaimed.

1241 North Seventeenth Street.

NOTES OF HOSPITAL PRACTICE.

PENNSYLVANIA HOSPITAL.

SERVICE OF R. J. LEVIS, M.D.

Reported by Dr. John B. Roberts.

TETANUS RESULTING FROM GUNSHOT WOUND OF THE ARM TREATED BY HYDRATE OF CHLORAL.—CONVALESCENCE OF THE PATIENT.

William T., aged 19, while gunning, was shot in the left arm by his own fowling-piece, the contents of the barrel tearing away the inner side of the arm and fore-arm for about six inches, and burying themselves in the tissues of the arm.

Upon admission, there was a good deal of hemorrhage from the wound, but the ulnar and radial arteries beating naturally at the wrist showed that the shot had not cut into the brachial artery. The wound was dressed with solution of carbolic acid and oil; but in a few days it assumed an erysipelatous condition, and the arm became much swollen and gave the boy severe pain. The treatment was continued, with the addition, however, of lead-water and laudanum locally, and iron and quinine internally.

Under this treatment the wound gradually assumed a more healthy appearance, large quantities of shot and wadding were discharged from its upper angle, the patient felt comfortable, and all things progressed favorably. On the fourteenth day, however, there came a change, and at the morning visit there was noticed a slight stiffness of the jaws, and some rigidity of the recti abdominis muscles, showing conclusively the advent of tetanus. The patient had great difficulty in eating, and, when asked to open his mouth, could scarcely separate his jaws, so that he could only receive liquid nourishment.

In a day or two these symptoms increased, so that the boy complained of pain in the posterior cervical region and along the spine, and of having bitten his tongue several times by the spasmodic snapping together of his teeth.

At the first intimation of tetanus the treatment adopted was ten grains of hydrate of chloral every two hours, with occasional doses of morphia when the pain was very severe. The object was to get the patient thoroughly under the influence of the chloral, and to keep him sleeping the greater part of the time. Accordingly, the frequency of administration of the drug was varied as circumstances indicated; sometimes the dose raised, at others the ten grains given every hour or hour and a half, while occasionally the interval was increased to three hours; the boy receiving, on an average, about one hundred grains during the twenty-four hours.

Under this treatment the patient was quite comfortable, and slept much, but there was no relaxation of the tonic spasm, and the muscular rigidity gradually increased.

Four days after tetanic symptoms first presented themselves, Dr. Levis, although the great gaping wound looked healthy, opened up the parts, in order to ascertain whether any wadding or shot yet remained imbedded in the tissues, and tore open the inter-muscular spaces with the finger, lest there should be irritating pressure exerted upon some of the nervous trunks of the arm. No foreign material was found, and, after the tension had been thus relieved, the limb was enveloped in a large poultice.

The symptoms continuing, it was thought that amputation might possibly afford a chance of relief, but, after consultation, it was decided that the removal of the limb at this period, after the nervous centres had been so certainly involved, was a step of too doubtful pro-

priety. Accordingly, the chloral-treatment was steadily persisted in, notwithstanding that diarrhœa occurred on the sixth day, whether or not produced by the drug it is difficult to determine. This looseness of the bowels was controlled by camphorated tincture of opium.

The trismus continued about the same, the dorsal muscles became more and more contracted until opisthotonos was very marked, and the patient became unable to empty his bladder. During this time, however, there were no spasms and no difficulty in deglutition, for the food readily passed into the stomach after it was placed in the mouth.

Traumatic tetanus seems to occur more frequently in young than in elderly subjects, and may follow the receipt of various injuries, often occurring after the most trivial. Dr. Levis has had the disease supervene after frost-bite, and even in a case where issues were made by Vienna paste for the cure of varicose veins.

In this case the tetanic symptoms first noticed in the masseter and temporal muscles did not present themselves until the fourteenth day after the receipt of the injury, though the affection often shows itself much earlier.

The face of the patient has an anxious expression, is moistened with perspiration, and the skin of the forehead is traversed by wrinkles; these signs constituting the characteristic physiognomy of tetanus. The muscles of the jaw are generally involved first,—whence the name of locked-jaw,—and then the posterior cervical muscles; afterwards the abdominal and dorsal, and finally those of the extremities, show rigidity; whereas the hands, and particularly the feet, seem to escape the tonic spasm.

In this case, owing to the chloral administered, the pain and stiffness increased gradually, without any positive tetanic spasms; although occasionally there were slight paroxysms in which he bit his tongue. For this reason he held between his teeth a piece of leather, to prevent injury to his tongue during the spasmodic snapping of his jaws. It is worthy of remembrance that it may be a dangerous experiment to ask a man with tetanus to protrude his tongue for inspection. A case recently occurred in the hospital in which the jaws shut down upon the tongue as it was thrust forward, and were separated only with great difficulty and after severe laceration of the member.

The prognosis of this, as of every case, was, of course, unfavorable, but when the patient had survived for two weeks since the advent of tetanus, it was thought that he might perhaps recover; for it has been frequently observed that the mortality is much less in those who have been kept alive until after the tenth day.

The treatment of this nervous affection is exceedingly unsatisfactory, and almost every remedy of the materia medica has been employed. Opium has long been considered the most efficacious; but so long as nearly every case of traumatic tetanus proves fatal, the search for some more potent remedy must be continued. Dr. Levis has seen three cases recover under the use of large doses of chloral, one under physostigma, and one under bromide of potassium in combination with chloral.

The chloral-treatment has been lauded so highly of late that it was thought well to test it in this case. Death occurs usually from asphyxia caused by laryngeal spasm, and it is probable that the chloral does nothing more than prevent the occurrence of spasms, and thus lessen the chances of suffocation.

* * * * * * *

At this time—five weeks since the trismus was first observed—the patient is convalescing, and, unless something unforeseen occurs, will steadily improve. There is little or no pain in the dorsal muscles, the opisthotonos is lessened, and he is able to separate his jaws wide enough to allow a thumb to be passed between the teeth; while the wound in the arm has filled up with granulations, is without pain, and rapidly cicatrizing.

Dec. 10.—The patient is now well.

ALLGEMEINES KRANKENHAUS, VIENNA.

CLINIC OF PROF. CARL BRAUN, OCTOBER 28, 1874.

GENTLEMEN,—We bring before you to-day a woman 35 years of age, who since yesterday has been in labor. Physical examination shows a pulse of 60, regular and strong; skin moist and cool, and the temperature of the body normal. On inspection, we find an abdomen much more than usually large; while a large tumor is found in the left inguinal region. The fundus uteri is strongly inclined to the right, and palpation reveals the uterus in a state of tonic contraction. What is here the cause of difficulty? Why does not the head pass into the pelvis and labor come to a natural and easy termination? A vaginal examination shows the pelvis rather more than usually small; but in every portion it is perfectly regular, and can by no means be termed a *contracted* or *narrow* pelvis. The cervix is very much elongated, and with two fingers introduced I am unable to reach the internal os. Meconium is present, a sign which we are often led to associate with difficulty or danger; and it is evident that the child is making fruitless attempts at respiration. On measuring the length of the child, which we can readily do by placing one hand over the fundus and the other over the head, upon the surface of the abdomen, or by its introduction into the vagina, making in all cases allowance for the intervening tissues, we find it to be about eighteen inches. Having ascertained its length, we can then estimate its weight; for the length of the child in utero and its weight, *as a rule*, bear to each other a definite relative proportion. The length of the child may be ascertained in the manner already mentioned as early as the fifth month. From this until the tenth month the child attains an increase in length of two inches for every month, and, if we reckon according to Austrian pounds, an increase of one pound for each month, so that we have such a table:

5th	lunar	month	= 10″ = 1	pound.
6th	"	"	= 12″ = 2	pounds.
7th	"	"	= 14″ = 3	"
8th	"	"	= 16″ = 4	"
9th	"	"	= 18″ = 5	"
10th	"	"	= 20″ = 6	"

An examination of the head, which we find to be the cause of the large prominent tumor in the left inguinal region, shows it to be very large, measuring certainly four or five inches in diameter. A vaginal examination also reveals the presence of very wide sutures, the sagittal suture measuring fully an inch in width. Here, then, we find the cause of this delayed labor, viz., hydrocephalus.

What, then, shall be our treatment, and what is our prognosis? Of one thing we are certain: this child cannot be born without the assistance of art, and hence we must make an attempt to help. Delay can bring no release to the mother, whose forces are gradually diminished as the period of delay is lengthened. If left alone, the case will terminate in one of two ways: either in metritis with fatal termination, or in rupture of the uterus; and the latter is most frequent when nature is unassisted. Shall we wait for the expansion of the os? No. As already said, the cervix is several inches in length; the internal os being situated opposite a point four inches above the mons; while the head stands on the brim of the pelvis, unable to enter. Turning is not practicable. The application of the forceps can

avail nothing; such an attempt would result in complete failure, and you would withdraw your instrument empty.

The treatment applicable in such cases, and which we will here make use of, is as follows: Introduce the left hand, grasp the head so as to hold it firmly in position, and then, passing a trocar along the palmar surface of the hand and fingers, so as to avoid injuring the tissues of the mother, introduce it into the cranial membranes, and withdraw the fluid. I prefer here a long and curved instrument, which will adapt itself to the curve of the pelvis. I would not, in such cases, perform craniotomy; not, however, because I think, as some assert, that any other course of procedure might lead to the birth of a child having the capability of living any considerable time. When not certain of the existence of hydrocephalus, you can apply the forceps, and make an attempt at extraction. It has also sometimes a good moral effect, while in the eyes of parents and friends it seems less barbarous than opening the cranium; and when you fail, as you must in hydrocephalus, you can proceed to puncture and deliver. The size of the head may be measured by the forceps. Introduce the blades, placing them in position, and measure the distance of the handles apart; then, removing, place them again in the same relative position to each other, and measure the distance between the blades. Applying that instrument, we find a diameter of four and one-half inches for the head. Introducing a trocar, we withdraw the fluid characteristic of hydrocephalus, a thin, transparent, watery liquid, measuring in this instance one and a half pints.

In these cases the great size of the head, still remaining after the withdrawal of the fluid, may offer a serious obstacle to delivery, and resort to the forceps or cranioclast is frequently called for. A resort to the cranioclast is always attended by a very unpleasant sensation to the operator. The movements of the child, indicative of the pain it suffers, can be distinctly felt, and it is no pleasure to know that you are thus destroying the life of a human being.

Since the withdrawal of the fluid, we find the relation of the uterus to the pelvis and the abdomen entirely altered. The head, which before presented such a large tumor in the left inguinal region, has receded; and the fundus, instead of lying so far to the right, has returned to the median line.

We here leave the case for the present to the efforts of nature, which it is hoped will prove all-sufficient for delivery during the lapse of a few hours.

G. Wilds Linn.

TRANSLATIONS.

On the So-Called Spontaneous Regeneration of Nerves.—Under this title Prof. Vulpian contributes to Brown-Séquard's *Archives*, Nos. 4 and 5, 1874, a communication, in which he modifies certain statements made by himself some years ago, relative to the regeneration of nerves.

Experiments were made in 1859 by Vulpian and Phillipeaux, who removed entirely the central part of certain nerves, so that it seemed impossible that any communication whatever could be established between this portion of the nerve and the peripheral segment.

It was found, however, that after becoming atrophied to a certain degree the peripheral portions of the nerves operated upon became the seat of an active process of regeneration. A large number of nervous fibres reappeared little by little, with all their normal characteristics. In certain nerves, as the hypoglossal, the regeneration at times seemed almost complete.

This took place although the hiatus established by excision between the central and peripheral portion continued complete. The researches of Waller had proved conclusively that every nervous fibre separated from its trophic centre would not only atrophy, but would never become regenerated unless put in connection with such trophic centre by reunion or reproduction.

"Our researches," remarks M. Vulpian, "seemed to prove that the propositions formulated by Waller had been too sweeping, and we concluded that the maintenance of the structure of nerves only depends in part upon the influence of the central nervous system;" "that altered nerves may become regenerated or restored without the intervention of an influence emanating from the nervous centres;" and, finally, "that nerves separated from the nervous centres may, even while they remain isolated from these centres, recover their normal structure and properties." "Although I believe that we took every precaution against error, I cannot go over the various details, but will confine myself to a few words relative to the experimental results which have led me to retract, on my part, today, the assertions formerly made by us relative to the autogenetic regeneration of nerves."

M. Vulpian then goes on to speak of certain confirmatory experiments. Having cut out a portion of the hypoglossal nerve of a dog, he found that after some months the motor power was still retained. He then made a deep incision at the initial end of the peripheric portion, sweeping the knife around so as to cut off any accessory filaments. On microscopic examination of this portion a week or so later, he found that, with the exception of a very few fibres, the nerve remained unchanged.

This seemed to prove almost conclusively the fact of autogenetic regeneration, since this regeneration could not be explained as apparent and depending upon anastomotic filaments, for these had been cut.

Further experiments, however, were made, which caused him to arrive at a different conclusion. When the peripheral portion was not only separated, but this process of isolation was carried some distance along its course, it was found that the motor power was almost entirely destroyed, and that under the microscope the nervous filaments were visibly altered. It seemed to result from these experiments that the peripheral portions of the hypoglossal or lingual nerves, after the exsection of a part of their length, only became regenerated because certain of the filaments and nervous tubes connected with these peripheral portions, and which had been cut during the operation, became united and reassumed their continuity. Anatomico-physiological relations were thus established between these nerves and the nervous centres by the intermediation of other nerves in the neighborhood.

The number of tubules in the anastomotic branches is always less than the number of tubules in the peripheric portions of the nerve which become regenerated. It is probable that each of the fibres which restore to the peripheral portion of the nerve the trophic influence of the nervous centres may give rise to several new fibres, or transmit this influence to a certain number of old fibres with the result of arousing in them the regenerative evolution.

If the new interpretation which is now proposed for these facts observed in 1859 is correct, it will be seen that the trophic influence of a nervous centre may be furnished to a nerve by another region of the gray substance than that from which that nerve takes its origin.

This proposition has already been proved by experiments made by MM. Vulpian and Phillipeaux on the end-to-end reunion of nerves of different origin.* It

* See Med. Times, April 25, 1874, p. 472.

seemed permissible to conclude from these experiments that the trophic influence of the nervous centres had been transmitted to the peripheric portion of the hypoglossal by the essentially sensitive fibres of the lingual nerve; and, as a consequence, that the region of gray substance where the sensitive fibres, those of the linguals, originate, may serve to maintain its integrity of structure of the fibres of the hypoglossal,—that is to say, of motor fibres.

Certain reservations, however, must be made in drawing conclusions from these facts, since the hypoglossal is far from being a simple nerve, and also because it anastomoses to a certain degree with adjoining nerves. The interpretation of these facts is beyond doubt very difficult.

M. Vulpian concludes: "Recent experiments which I have made in the regeneration of the peripheral portion of the hypoglossal after exsection of the central part of this nerve enable me to assert that this regeneration does not take place in an independent manner and without any influence on the part of the nervous centres. It appears to come about by the mediation of fibres, anastomotic or other, which, in regaining their continuity, place the peripheral portion of the hypoglossal nerve in anatomico-physiological relation with these centres."

The same explanation would answer quite well for all the other facts of so-called autogenetic regeneration observed after removal of the central portions of the facial or spinal nerves, etc.

Certain nerves, as the sciatic, having few anastomoses, become regenerated but slowly, and at best but to a slight degree; even some months after exsection of a portion of the sciatic in a rabbit, only a small number of regenerated nerve-tubules were found in the peripheral portion of this nerve.

The laws of Waller, therefore, reassume their entire and absolute value; and in nerves, the nervous fibres provided with myelin do not preserve or recuperate the integrity of their structure during extra-uterine life, and above all in adult animals, but on the condition of being in relation, the one (sensitive fibres) with the ganglions of the posterior root, the other (motor fibres) with the gray substance of the cerebro-spinal axis.

A. V. H.

Preventive and Exploratory Trepanning in Fractures of the Internal or Vitreous Table of the Cranium.—M. Sédillot read a note on this subject before the Académie des Sciences at a recent meeting. His conclusions were as follows:

1. Preventive trepanning is the surest method of treating all fractures of the internal table of the cranium complicated by splinters.

2. The operation is demanded in cases of external stellate or linear fracture with depression of the cranium.

3. Hesitation is permissible in simple linear solutions of continuity without displacement of the bones.

4 The means of diagnosis comprise the causes of the traumatism, symptoms, auscultation, percussion, thermometry, and of explorative trepanning.

5. The absence of any exterior fracture does not contra-indicate the possibility of an internal one, following direct traumatisms of a circumscribed or violent nature; and if auscultation and percussion, the force of the blow, the nature of the wounding body (gunshot wounds) do not indicate the necessity of exploratory trepanning to the surgeon, it is his duty to watch the patient carefully, and to have recourse to this operation without delay, where new symptoms indicate the opportunity. This is understood to be in localities exempt from infectious influences which experience shows to be constantly fatal in similar cases.

6. Precautions and dressings founded upon the theory of ferments will modify, perhaps, this inability to operate, and seem already to promise most favorable results in wounds of the cranium.—*Gaz. Méd. de Paris*, Oct. 24, 1874. A. V. H.

Chloral as an Application in Gangrene.—M. Dujardin-Beaumetz communicated to the Société des Hôpitaux, at a recent sitting, an account of the case of a young man 18 years of age, who applied for relief for spontaneous gangrene affecting the hand and fore-arm. In order to dissipate the odor and to avoid the danger of septicæmia as far as possible, M. Beaumetz had the affected limb kept constantly in a bath containing one part chloral to five hundred parts water. At the end of eight days the arm was amputated, with a favorable result. A. V. H.

Congenital Absence of the Bladder—Incontinence of Urine—Catheterization—Death from Peritonitis.—At a recent meeting of the Société de Chirurgie, M. Gyon read for M. Fleury the following observation:

A young girl who had menstruated since about a year previously, entered the hospital at Clermont for incontinence of urine of some months' duration. The vulva and the upper portion of the thighs were the seat of a painful erythema. A sound introduced into the canal of the urethra did not penetrate more than about an inch and a quarter. The vaginal touch showed relaxation of the vulvar ring, but furnished no further information.

Her general condition was good. Examination with the speculum was postponed until the following day. At that time the patient complained of pain in the belly; the skin was hot, the pulse frequent. The catheterization had been carefully made, and these symptoms could not be accounted for. They increased in severity, and the patient succumbed a few days later.

The autopsy revealed an effusion of pus into the abdominal cavity, covering the folds of the intestines. The bladder was entirely absent, the ureters terminating on the sides of the urethral cul-de-sac. The left kidney contained creamy matter of a whitish-yellow color; the right kidney was healthy. It was difficult to explain how, with such a vice of constitution, the incontinence dated back only one year; later, however, M. Fleury learned from the patient's mother that this infirmity was congenital, but that it had become insupportable only within the last year. It is possible that within this time the urine had acquired an acridity which brought on the erythema.—*Bull. Gén. de Thérap.*, Nov. 15, 1874. A. V. H.

Hydatid Cyst of the Liver relieved by Aspiratory Puncture.—At a recent meeting of the Société des Hôpitaux, M. Dumontpallier communicated the following case:

A patient, after having presented for some time the various symptoms of dyspepsia, showed signs of a chronic pleuritic effusion on the right side. A series of blisters were applied, and aspiratory puncture was proposed,—the diagnosis of hydatid cyst of the liver having been arrived at. The proposal of this treatment was negatived at the time by the patient and the consulting physicians. Some time afterwards the dyspnœa became very distressing; puncture was again proposed, and was practised with a No. 2 Potain's trocar, about half an inch below the nipple. The exit of a transparent, non-albuminous liquid, containing the débris of hydatid sacs, was the result. The operation was followed by immediate relief, and resulted in complete cure.—*Bull. Gén. de Thérap.*, Nov. 15, 1874. A. V. H.

PHILADELPHIA
MEDICAL TIMES.
A WEEKLY JOURNAL OF
MEDICAL AND SURGICAL SCIENCE.

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EDITORIAL.

THE RIGHT MAN IN THE RIGHT PLACE.

THE pressure of original matter upon the columns of the *Times* is at present so great that we must defer considering the causes of the slow growth of specialism in this city; but we desire briefly to call attention to the recent appointment of coroner's physician, as an instance of a tendency to the belief that the less education upon a special subject a physician has, the more fit is he for a position involving special knowledge of that subject; or else of an almost equally indefensible tendency to select men not because they have special fitness, but because they are friends of the appointing power, or have some coveted influence.

Dr. Goddard, our new coroner, was elected largely through the efforts of the profession, because a medical coroner was wanted, and because it was believed that he would appoint such physician or physicians as, having a knowledge of medical jurisprudence, would gather important gleanings from their experience to add lustre to the medical fame of our city. We have a few men of ability who have devoted a great deal of attention to the subject of medical jurisprudence; but, instead of selecting them, Dr. Goddard has appointed two of our busiest and most popular surgeons,—men of the highest eminence in surgery, but destitute of any special training in, or knowledge of, medical jurisprudence,—men, too, so absolutely engrossed in public and private practice that it is absurd to suppose they can study up the subject, or even attend to the mere routine duties of the position; of course the work will be done by deputies.

Dr. Goddard, knowing that his appointees are deservedly esteemed as surgeons, may have chosen them because he thought that they could influence the profession in his favor; but we think the Philadelphia profession will never stultify itself by endorsing such an appointment. If Dr. Goddard had selected Prof. Reese, or some other equally well-known special student of medical jurisprudence, he would have greatly strengthened himself: as it is, when, in the future, he wants most the sympathy and aid of his brethren, he will perhaps discover his mistake.

LEADING ARTICLES.

THE TRANSMISSIBILITY OF TUBERCULOSIS.

II.

IN our last article it was shown that tuberculosis may be communicated by inoculation. A still stronger corroborative proof that it is a virulent infectious malady is furnished by those investigations which have been made to demonstrate its wellnigh infallible communicability through the digestive canal.

It has now been settled beyond all controversy that the contagia of certain virulent diseases, when administered, can infect as readily through the digestive organs as through any other channel. This is especially true of variola crusts and pustules, and of the discharges from glanders and anthrax.

Chauveau, of the Lyons Veterinary School, with a view to ascertain whether phthisis was also transmissible in this manner, instituted a series of experiments by administering large quantities of tuberculous material. Deeming it essential that a species of animals should be procured in which tuberculosis was a spontaneous disease, endowed with the characters it presents in man, he therefore selected the bovine species, and, as a precaution to avoid experimenting upon animals already infected, he resorted to calves born and bred beyond all those influences or conditions which favor the natural development of phthisis.

Four calves were selected. One, which was kept for comparison, remained unaffected; the three others, fed upon tubercular matter, were in such a miserable state fifty-two days from the commencement of the experiment that it was considered time to kill them. The autopsies revealed the most perfect lesion of generalized tuberculosis, the mesentery and intestines being extremely involved. Peyer's glands were nearly all in a state of ulceration; besides, there was an eruption of tubercles throughout from the duodenum to near the termination of the colon. The lesions in the thorax were less marked than those of the abdomen; all the

bronchial and mediastinal glands were enlarged and diseased. Throughout, there was a marked predominance of the glandular over other forms of tuberculosis. To show that there was no analogy between this induced disease and purulent infection, Chauveau states that he has introduced with impunity large quantities of pus into the digestive canal. He concludes his important communication with this résumé of his experiments: 1. They prove that bovine animals contract tuberculosis by digestive ingestion, as they may take anthrax and vaccinia. 2. They place beyond doubt the fact as to the virulence and contagious property of tuberculosis, and show that the labors of Villemin have not been recognized as they deserve. 3. They show that the digestive canal constitutes in the bovine species, as in man, a channel of contagion readily disposed for the propagation of tuberculosis, and which may be more frequently the mode of access than through the pulmonary organs.

Gerlach, Böllinger, Zürn, Leisering, Harms and Gunther, Klebs, Bagge, Saint-Cyr, Pétroff, Vergad, besides other experimenters, have all succeeded in producing artificial tuberculosis by feeding tuberculous flesh and lungs, caseous lymphatic glands, etc., taken not only from phthisical men, but also from cattle and pigs spontaneously or artificially affected with the malady. The different varieties of animals chosen for these experiments were nearly as numerous as the varied investigators, so that now, besides rabbits and guinea-pigs, sheep, pigs, dogs, cats, and cattle may safely be said to be susceptible of acquiring tuberculosis by ingestion in the digestive canal. This long list of carefully-conducted experiments certainly justifies the conclusions as to the virulency of this affection, and must appear convincing to any but the most skeptical.

Of these investigations, those which were made with the cooked and uncooked flesh and milk of phthisical cattle are by far the most interesting and important in their practical bearings.

Böllinger, Gerlach, and Klebs, especially, have been successful in producing tuberculosis by giving animals milk from phthisical cows. In addition to rabbits and guinea-pigs,—creatures apparently very susceptible to the artificial production of the disease,—Klebs* accidentally induced the disease in a dog by feeding it with the milk of a cow in the last stages of phthisis. The use of this milk seems always to produce tuberculosis, which commences as an intestinal catarrh, and then gives rise to mesenteric glandular tubercles; later it affects the liver and spleen, and finally the thoracic organs. He thinks it probable that the virus of tuberculosis may exist in varying proportions in the milk of phthisical cows according to the extent of the disease in them; and he further believes that the malady may be developed in children born without any tendency to it, through the medium of the milk of the mother or nurse.

Böllinger thinks it probable that intestinal tuberculosis, consecutive to pulmonary phthisis, may be produced by the sputa being swallowed and passing into the intestine.

The present status of the transmissibility of tuberculosis is admirably summarized in Gerlach's statement of the results of his experiments. 1. The tuberculosis of cattle is very infectious. 2. The tubercles covering the serous membranes, as well as those in the other organs, are as infective, and produce the same tubercles, as the tuberculous matter of the lungs. The identity of pulmonary phthisis of cattle and general tuberculosis cannot be doubted. 3. Infection can be produced after inoculation as well as after ingestion of the tuberculous matter. 4. The *flesh* of animals affected with tuberculosis possesses, in certain conditions, the power of infecting, though to a less degree than tuberculous matter. 5. The temperature of boiling water destroys the infective principle, though boiled tubercles, nevertheless, often preserve a certain degree of virulence. It is in this as in trichinosis. 6. The infective properties of uncooked phthisical milk can no longer be doubted or denied.

Thorough cooking of the milk and flesh, it is true, may completely destroy their virulent properties; and even though flesh in itself is hardly to be regarded as contaminating unless it contains infiltrated lymphatic glands, yet, as we all know that tuberculized viscera, when they are not much affected, are not rejected as food, we must, in the light of all these experiments, regard these articles as playing an important part in the propagation of tuberculosis in the human species. To enumerate here the prophylactics against such possible sources of contamination would be superfluous; but does it not behoove our sanitarians and legislators to adopt the necessary preventive measures?

Another form in which infectious phthisical matter may enter the economy through the digestive system is that of the sputa, either in the moist or dried condition. Veterinarians have long believed and admitted this source of infection among cattle. That the dried mucosities and secretions are capable of transmitting the disease among animals scarcely needs any further proof than the evidence already furnished. Upon this point the observations of M. Grad are very conclusive. When visiting the stables of an extensive farmer at Leinheim, he was informed that every year for five years one of the cattle had died from phthisis; "and, what is very curious," said the farmer, "this always happens in the same stall." Believing that an hereditary tendency was doubtless the efficient cause in this succession of losses, another perfectly healthy animal was placed in the same stall, which in time presented all the evidences of the disease. The conclusion being now made that, in all probability, the disease had been transmitted by the ingestion of the matters expectorated by the animals which had previously inhabited the same place, all the wood-work of the stall, manger, and rack was removed, and the place thoroughly disinfected. It is significant that, although the stall was rebuilt and successively occupied, tuberculosis made no more victims in it.

* Klebs, "Ueber die Entstehung der Tuberculose und ihre Verbreitung in Körper," Virchow's Archiv, Bd. xliv. S. 242.

So great is the danger of transmission of tuberculosis among cattle that it is considered advisable to isolate completely those that are diseased from the healthy animals. For ourselves, we do not understand how, in the face of all these facts, it can any longer be said that the human species are exempt from these same dangers of infection. R. M. B.

CORRESPONDENCE.

To the Editor of the Philadelphia Medical Times:

New York, November 25, 1874.

THE meeting of the Medical Society of the County of New York last evening was one of unusual interest. The retiring President, Dr. Elsworth Eliot, gave a brief valedictory address, in which he alluded to the history and progress of the Society during the past year, referred in an appropriate manner to those who had been retired from the ranks of membership by death, and gave a stinging rebuke to that class of regular physicians who make known their business and qualifications to the people at large by means of advertisements in the newspapers,—one of whom had been ignominiously and promptly expelled during the year. He also referred to the fact that while certain medical societies in the city—he mentioned the Medical and Journal Association—were in the habit of inviting members of the daily press to be present, so that reports of their proceedings were frequently found in the newspapers the next morning, this practice did not hold good in regard to this Society.

He touched upon the question of the medical law of this State, as passed by the Legislature at its last session, which compels every medical graduate in this city to register, and every man or woman practising medicine in this city who does not hold a diploma, to pass an examination before a properly appointed examining board.

While the law does not cover the ground so fully as it is wished, yet, if the community by its enforcement are the gainers, it will not have been passed in vain. The censors have exercised the utmost vigilance; and they are not disposed to let down the bars so that designing men can claim equality in rank with the regularly qualified physican. He adverted to the action of the Board of Public Charities and Corrections, and thought that the rule granting equal clinical advantages in hospitals to the several medical schools would work to the advantage of medical education and the community at large. Dr. Eliot did not favor the practice of giving to specialists a particular designation,—as, for instance, to him who practises exclusively upon the eye, the name oculist. He preferred and recommended the manner of designation as follows: "Geo. Blank, M.D.; special attention given to Diseases of the Ear," etc.

The term "Doctor," in medicine, should apply to all graduates. The variation should consist in something after the above form.

Dr. Eliot then retired from the chair, and Dr. Sands, the President-elect, assumed control as presiding officer for the ensuing year; and, in doing so, he called attention to an invitation to the members of this Society to meet the Academy of Medicine to consider the subject of the erection of a building to be devoted especially to the meetings of the societies of the city,—or, in other words, the founding of a home for the profession in this city.

Prof. Sands then proceeded to deliver his inaugural address, the principal object of which was the consideration of Esmarch's method of bloodless operation, and the results of the same as regards the city of New York and vicinity during the last year.

He first considered briefly the history of the operation; secondly, its advantages and disadvantages; and thirdly, an estimate of the value of the operation according to the experience of our own surgeons. He also described the method of the application of the bandage. He prefers an elastic bandage to the cord, because the latter will be apt to injure the vessels and nerves, while the former, on account of the surface over which the pressure is extended, is less liable to this result.

As regards the operation in this city, Prof. Sands presented a table of cases collected from the principal hospitals of New York, Brooklyn, and Jersey City, and from the private practice of Drs. Wood, Krackowizer, Gouley, Ely, Weber, Little, and himself. The records of the Charity Hospital are not included, as they were so imperfectly kept that no use could be made of them. He also failed to obtain the experience of St. Francis's Hospital, in this city, where more than twenty bloodless operations are said to have been performed. He had endeavored to procure all of the hospital cases, both good and bad; and, although some of the histories are imperfect, they present many interesting and instructive details. The list embraces one hundred and forty-three cases, including 63 amputations, excluding those of the fingers and toes. Of these operations 44 were primary, for injuries, and 19 secondary, or for disease. Of the primary amputations 10 cases, or 22.7 per cent., terminated fatally, the causes of death being as follows: pyæmia, 4; exhaustion or shock, 3; spreading gangrene, 1; hemorrhage, 1; erysipelas, 1. Of the cases fatal from pyæmia, one patient had the disease at the time of the operation. Of 8 cases of primary amputation of the thigh, 2, or 25 per cent., ended fatally. Of 11 cases of primary amputation of the leg, 3 cases, or 27.3 per cent., were fatal.

Secondary amputations—19 cases—give us a mortality of 21 per cent. The causes of death are stated as follows: tetanus (existing at time of operation), 1; exhaustion, 1; phthisis, 1; pyæmia, 1: total, 4.

Although he had not the means of making an exact comparison, he believed that the percentage of recoveries in these primary amputations—which were nearly all performed upon hospital patients—is somewhat larger than is commonly observed in such operations.

Of sixteen cases of excision of the joints, only one

proved fatal, the cause of death being pyæmia; another case will probably end fatally, however, as when last heard from the patient was reported to have tetanus. Finally, the list includes two more deaths: one following an operation for necrosis,—the only fatal case out of 36,—and the other an operation for ligation of the ulnar artery, performed for a wound near the wrist.

Upon the whole, the method is one concerning which he spoke in the highest terms. It is one of the most valuable surgical expedients of modern times, and needs only proper knowledge and skill in its application.

Prof. James R. Wood, M.D., remarked that he had operated twenty-five times, each time employing this method, without bad results, with the exception of one case, mentioned in the table of Dr. Sands. He has repeatedly operated, both in hospital and private practice, in young and old, men and women, and has had no trouble whatever, and he has "fallen in love with the operation." The surgeon who formerly performed his operations by groping in the darkness can now witness each step of the operation with the eye. "Is there not," said he, "something beautiful in this,—something worthy the attention of surgeons?" He was not in favor of the cord, but of the bandage.

Prof. Frank H. Hamilton has made eight operations employing the bloodless method,—four amputations and four exsections,—and he is highly pleased with the success attained. He has had no accident that he can possibly attribute to Esmarch's method. He did not look upon the cord with favor. He preferred a bandage.

Prof. Markoe was satisfied with the efficacy of the method in preventing hemorrhage. He has had no accidents from this cause. He mentioned a case where the cord had been used. There was no sloughing at the time, but five or six weeks after the operation, just at the point where the cord had made pressure, a sloughing occurred down or nearly down to the bone. The case, however, recovered. With this single exception, he had experienced no bad results.

Dr. Krackowizer, who was the first to employ this method in this city, on the 22d of October, 1873, is still in favor of it. Instead of the rope or elastic webbing or bandage, which is objectionable on account of the possibility of its accumulating filth, and because of the inability of the surgeon always to secure the most approved articles, he thought it well to extemporize a bandage whenever practicable. He had from the first objected to the cord, and was in the habit of splitting a common india-rubber hose, an inch or so in diameter. This served the principle and the purpose.

Dr. Watson spoke of a case occurring in the town of Niagara, N.Y. The wrist was crushed. Operation was performed at the lower third of the fore-arm, by Esmarch's method. On the second day after the operation, the tissues became somewhat discolored. On the fourth day the limb in the neighborhood of the stump became gangrenous. The unfavorable character of the case rendered a second operation necessary. He has not heard further from the case.

Dr. Parker had not had any personal experience in the matter, but suggested in a general way various untoward results which might possibly happen.

To sum up, the sentiment of the meeting appeared to be largely in favor of this method. It was looked upon as second in importance, as relates to the whole field of surgery, to the discovery of anæsthesia; and, like the subject of anæsthetics, it requires close and extended study to bring it to a state of perfection. W.

REVIEWS AND BOOK NOTICES.

THERAPEUTICS AND MATERIA MEDICA. By ALFRED STILLÉ, M.D. Fourth Edition, thoroughly Revised and Enlarged. H. C. Lea, Philadelphia, 1874.

The magnificent work of Prof. Stillé is known wherever the English language is read and the art of medicine cultivated,—known so well that no encomiums of ours could brighten its fame, and no unfavorable criticism could tarnish its reputation. Its peculiar merits and its peculiar faults—the individuality of the book, so to speak—have long since been generally recognized, so that it is our only duty to announce the appearance of this new edition, and to assure our readers that it is, as it were, an exaggeration, or an intensification—pardon the word—and amplification of the familiar volumes. So much more has been gathered into this vast storehouse of the universal clinical observation that the reader will be more embarrassed than ever by the very mass of matter. Two hundred and fifty new pages indicate the amount of this addition.

We are very far from desiring to criticise the volumes before us. As an American we are proud of them, and as a Philadelphian we are doubly so, but we cannot refrain from making a few remarks upon a paragraph in their preface.

Dr. Stillé says that in the first edition of the work he contended against "the mischievous error of seeking to deduce the therapeutical use of medicines from their physiological action. Continued study, observation, and reflection have tended to strengthen his convictions upon this subject, and to confirm him in the faith that clinical experience is the only true and safe test of the virtues of medicines." We are not exactly sure of the meaning which Dr. Stillé intends to convey by these words. If he means simply that clinical experience guided by a thorough knowledge of the physiological action of remedies and of the pointings of nature in disease is the ultimate test in therapeutics, we know of no one who will disagree with him. But if he means that therapeutics is to be advanced solely or chiefly by what we may be pardoned for calling the blind, unlettered experience of mere clinicians, who are not acquainted with the physiological action of remedies, and even despise such knowledge, we most earnestly dissent. If there be no better method of therapeutic inquiry than that so long employed, the future looks to us almost hopeless, and we welcome the quiet calm of that therapeutic despair—the so-called expectant method of treating diseases—in which patients are left to the sole care of nature and of hygiene. The work of Prof. Stillé itself—we speak with the utmost respect—seems to us to afford the strongest evidence of the utter failure of purely clinical therapeutics. How any one can look over the index of "Therapeutics" and see the long list of the most diverse medicines used in various individual diseases, and still retain his faith, is more than we can understand. Thus, in rheumatism there are between forty and fifty more or less antagonistic remedies. Or, as it may be objected that rheumatism is very variable in its type, let us examine tetanus,—of all

diseases the most constant in its causation, its type, and its symptoms. All of the substances referred to in the index are mentioned in the text as having been found useful in the experience of some one; on very few of them does Dr. Stillé give any opinion based upon personal experience, and, indeed, very frequently he abstains from expressing any opinion whatever. This is, then, the index list: calabar bean, strychnia, aconite, alcohol, cold, heat, nitrite of amyl, wine, tobacco, ether, garlic, belladonna, tartar emetic, chloral, cannabis indica, quinine, colchicum, chloroform, electricity, bromide of potassium, and opium, besides counter-irritation with various substances. In other words, the greater part of the officinal strongest arterial stimulants and the strongest arterial sedatives, of the strongest spinal sedatives and the strongest spinal stimulants, the adding to the system and the taking from the system of the diverse forces of nature, nearly all of the officinal drugs which most profoundly affect the sensorium, the most virulent antagonistic poisons and the most trifling of drugs, all are recommended by experience for the cure of tetanus! Verily, the profession must be worse than a blind man led by the blind, if it does not begin to doubt this infallible guide,—experience. If eighteen hundred years of labor have only this to offer as their fruit, is it not time that we should mistrust the method?

Erysipelas and Child-bed Fever. By Thomas C. Minor, M.D. Robert Clark, Cincinnati.

The perplexing subject of puerperal fever ever and anon attracts a new writer, who is fortunate if he flutter away again with unsinged wings. The little volume before us—one of the latest contributions to the literature of this disease—is an examination of American statistics, aiming to discover what connection, if any, exists between puerperal fever and erysipelas. To many this immediate question seems already decided, and yet all are conscious that about this fever there is a mystery still unsolved. The evidence seems conclusive enough that a disease, called in the mortuary reports of cities and in the statistics of hospitals puerperal fever, is prone to coexist with another disease called erysipelas; but until the varied forms which are comprehended under these names are strictly described, analyzed, and made distinguishable by the average professional mind, the matter will be likely to remain in its present unsatisfactory state. While some observers contend that all cases of child-bed fever are cases of septicæmia, others as decidedly deny it, and argue for the existence of a distinct specific contagious and infectious principle, as distinct as that supposed to exist in the case of typhus and scarlatina; while some levellers would merge all contagions into one universal destructive agent, which, modified by circumstances, produces scarlatina, typhus, erysipelas, etc., at its own sweet will.

Dr. Fordyce Barker, in his recent admirable work on "The Puerperal Diseases," states his belief in a puerperal septicæmia, scarlatina, erysipelas, etc., as distinct and different from true puerperal fever itself,—a disease which he claims to have seen and recognized before labor, in cases where septic infection was impossible; epidemic causes, contagion and infection, producing the true disease; autogenetic infection and direct inoculation or nosocomial malaria, its spurious but equally deadly ally, puerperal septicæmia. The statistics collected by Dr. Minor embrace those of the various States and Territories—in fact, all the statistics of all the States accessible—for all the years recorded,—public statistics in our favored land being as yet in their infancy,—with as many accompanying conditions—elevation, temperature, humidity, season—as possible.

The work, it will be seen, therefore, is one of great value for reference, though the general conclusion reached by the author is guarded, and such as might be expected, viz., that "erysipelas and puerperal fever seem to prevail together throughout all the States." The author says, in conclusion, that he has at least "the satisfaction of knowing that many pleasant hours have been whiled away in the attempt to throw light on a somewhat obscure point."

GLEANINGS FROM OUR EXCHANGES.

Dolbeau's Operation for Stone in the Bladder —Perineal Lithotrity (*The Lancet*, October 17, 1874).—Mr. William Ewart calls attention to an operation for the extraction of vesical calculi, which, though really a revival of the old Marian method of "small incision, much dilatation," is also a great improvement upon it, inasmuch as the passage which previously resulted from the laceration of the tissues is now formed by their simple and equable compression. The stone is almost always crushed previous to its removal, whence the name of "perineal lithotrity" given to the operation.

The instruments used are few: a scalpel, a screw dilator, a crushing forceps, and various-sized seizing forceps. The essential feature in the screw dilator is that the blades keep parallel in every stage of their expansion, forming together a cylindrical body, the diameter of which varies with the action of the screw. At rest, the six smooth blades or columns are in apposition, and run up to a common point. They conceal two balls capable of moving up and down along a central stem, and of separating the columns more or less without causing them to diverge. The expanding portion of the instrument measures eight centimetres in length, one and one-fifth centimetres in its smallest diameter, and two centimetres in its largest; the limit of distensibility of the neck corresponding to a diameter of two and two-fifths centimetres. The forceps are of great power, and are so constructed that they can be opened after their insertion without any divergence of their limbs taking place in the thickness of the perineum.

The patient is placed in the usual lithotomy position. The lower limbs are intrusted to two assistants; a third (whose office is responsible) takes charge of the strongly-bent and deeply-grooved staff; his duty is to keep it strictly in the middle line, to make its curve project in the perineum, and later to resist the pressure of the point of the dilator. The incision, two centimetres in length, is made with the blade upwards, and begins immediately in front of the anus, at the brown zone marking the passage of skin into mucous membrane. A deeper incision opens up the perineal fascia, through which the knife, guided by the left index-finger, is thrust into the membranous portion of the urethra. The work of dilatation now begins; and upon the amount of care with which it is accomplished depends in a great measure the success of the operation. Four successive dilatations are employed by Professor Dolbeau. The first stretches the external aperture and subcutaneous tissues. The instrument is next closed, and passed up to the groove of the staff and firmly pressed against it whilst the expanding screw is being turned,—a proceeding that causes the whole of the membranous portion to slit up along the line of incision. The instrument can now be pushed one step farther along the staff, which the assistant slightly depresses. As soon as the membranous region has sufficiently yielded to this third dilatation, the dilator is closed and pushed onward into the neck of the bladder, the staff is withdrawn, and the screw cautiously turned until expansion is complete. The passage thus formed is of uniform calibre, and has smooth walls. This can be felt by the

finger in the dead subject. In the living, Prof. Dolbeau deprecates the use of digital examination. The stone is now within easy reach, and if very small will be removed by the light forceps used in making the first exploration after removal of the dilator; generally, however, it has to be broken up by the casse-pierre. This is introduced without any great difficulty, and, thanks to the broad blunt beak in which it terminates, it obtains an easy grip even of the harder and smoother concretions. Large stones cannot be clasped, but will usually yield to the repeated bites of the instrument. The fragmentation of the stone is often very great; and to extract everything, even with the help of injections, is a task requiring much patience. Before the patient is released from the lithotomy posture, a catheter is always introduced through the natural channel to obtain a crucial proof of the bladder having been cleared. The loss of blood is trifling, and seldom exceeds two or three teaspoonfuls; the wound is minute:—two important considerations in dealing with old age and albuminuria. The bulb is out of the way of danger, and the rectum is not seriously imperilled. The grave surgical complications of pyæmia and of urinary infiltration have been hitherto unknown.*

The patients are generally enabled to leave their beds soon after the operation, often as early as the second or third day.

The operation itself is, however, a long one; it must be conducted slowly to be safe, and of course this necessitates a prolongation of the anæsthesia. It is also difficult, and requires to be studied in all its details, and practised repeatedly, before it can be safely attempted.

NOTES AND QUERIES.

GEORGE LABAR, THE POST-CENTENARIAN.

We briefly chronicled, yesterday, the death of George Labar, of Monroe County, Pennsylvania, which occurred on Saturday last. It was the close of one of those wonderfully prolonged lives which run far back beyond the memory of other living men. George Labar was born in the autumn of 1763, *one hundred and eleven* years ago, at Mount Bethel, Northampton County, now called Portland, where his baptismal register still exists. He was of French descent, his grandfather settling in Pennsylvania in 1730, a fugitive from religious persecution.

He spent his whole long life among the scenes of his nativity, and retained to the last vivid recollections of the old Indian wars, the Revolution, and the subsequent early events of American history. Nearly seventy years ago he accompanied his parents in their emigration to Ohio, but only remained with them a few days, and returned home. His father lost his wife when he was ninety-eight years old, and the gay old gentleman married again at one hundred! He died at one hundred and five, and was buried in Ohio.

George Labar, who has just died, has preserved a wonderfully hale and hearty condition to the last. He was of medium height, had a large head, well covered with snow-white hair, a keen, quick eye, and a face of strongly-marked features, not much marred by age. In walking, he stooped considerably, but was possessed of sufficient vigor to walk some rods without assistance. Like all centenarians, he remembered General Washington,—in this case there is no improbability in the narration,—and talked enthusiastically over the time when the news of Washington's capture of the Hessians at Princeton spread up the valleys and brought joy to so many loyal hearts. He thought Washington had a charmed existence, and that bullets were powerless to harm him. Once he saw the great general at Easton, then an insignificant hamlet, and he said Washington wore buckskin breeches, silk stockings, and silver shoe-buckles. This aged relic of a former century related these particulars with a zest and earnestness which strongly persuaded one of their truthfulness.

George Labar had never been sick but three times in his life: once with yellow fever, once with camp-fever, and once with typhoid fever. He used tobacco very freely all his life, both smoking and chewing, but was very moderate in his use of liquors of all kinds. He took daily exercise in the woods, among which he had grown up, felling trees and chopping railroad-ties up to within the last two or three years. It is recorded of him that in 1869, when one hundred and seven years old, he felled trees and peeled three wagon-loads of bark, which one of his youngest sons, a young chap of sixty, hauled to market for him.

George Labar was married in 1788, and leaves a large number of descendants, who are scattered around Monroe County. His oldest son was born in 1791, and married, at twenty-one, a girl of thirteen! Their oldest child is now sixty, and is only fifteen years younger than his mother. In 1870, George Labar had living a sister aged eighty-six, another sister aged ninety-two, and a brother, in Canada, aged ninety-eight, a brisk old boy, who paid George a visit in 1868.

One of the most remarkable features of Labar's longevity is the fact that he voted for General Washington for President, and has voted at every Presidential election since, *and always for the Democratic candidate!* And yet he lived to be one hundred and twelve years old.

George Labar was one of the best-attested cases of extreme longevity on record, and, while his extraordinary prolongation of life attests the value of a good constitution lived out in a rugged experience of out-door life, it presents quite a serious difficulty in the way of the absolute doctrine of the anti-tobacco and whisky theory.—*Philadelphia Evening Bulletin*, October 1.

* In none of Prof. Dolbeau's cases did the prostate resent the pressure to which its tissues had been subjected; and the neck of the bladder always escaped rupture or over-stretching of its fibres.

OFFICIAL LIST

OF CHANGES OF STATIONS AND DUTIES OF OFFICERS OF THE MEDICAL DEPARTMENT U.S. ARMY, FROM DECEMBER 1 TO DECEMBER 7, 1874, INCLUSIVE.

Sternberg, G. M., Assistant-Surgeon.—Granted leave of absence for one month. S. O. 196, Department of the Gulf, Dec. 1, 1874.

Gibson, J. R., Assistant-Surgeon.—Granted leave of absence for fifteen days, upon completion of his examination for promotion. S. O. 260, A. G. O., Nov. 30, 1874.

Phillips, H. J., Assistant-Surgeon.—Assigned to duty at Fort Ontario, Oswego, New York. S. O. 236, Military Division of the Atlantic, Nov. 30, 1874.

Heizmann, C. L., Assistant-Surgeon.—Relieved from duty in the Department of the Platte, to report in person to the President of the Army Medical Board, New York City, for examination for promotion, and, upon its completion, by letter to the Surgeon-General. S. O. 260, A. G. O., Nov. 30, 1874.

By S. O. 260, War Department, November 30, 1874, the following-named Assistant-Surgeons (recently appointed) are directed to report as follows for assignment to duty:

Winne, Charles K., in person to the Commanding General, Department of the Platte.

Ainsworth, F. C., in person to the Superintendent United States Military Academy, West Point, New York, for temporary duty.

Havard, Valery, by letter to the Commanding General, Department of Dakota.

Hoff, John Van R., by letter to the Commanding General, Department of the Platte.

Paulding, H. Offley, in person to the Commanding General, Department of Dakota.

Adair, George W., in person to the Commanding Officer, Newport Barracks, Kentucky, for temporary duty.

Brown, Paul R., by letter to the Commanding General, Military Division of the Atlantic.

Moseley, Edward B., by letter to the Commanding General, Department of the South.

Jackson, Donald, in person to the Commanding General, Department of Texas.

Semig, Bernard G., by letter to the Commanding General, Department of California.

Skinner, John O., in person to the Commanding General, Department of the Columbia.

Finley, James A., in person to the Commanding Officer, Fort Monroe, Virginia.

De Loffre, Augustus A., in person to the Commanding General, Department of the Missouri.

Bedal, Sylvester S., by letter to the Commanding General, Military Division of the Atlantic.

Hamilton, John B., in person to the Superintendent of Mounted Recruiting Service, St. Louis Barracks, Missouri, for temporary duty.

Wilcox, Timothy E., in person to the Commanding General, Department of the Missouri.

Maus, Louis M., by letter to the Commanding General, Department of the South.

Moseley, E. B., Assistant-Surgeon.—Assigned to duty at Columbia, South Carolina. S. O. 197, Department of the South, December 4, 1874.